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NOTES ON RECENT WORK IN THE PHILOSOPHY OF SCIENCE.

Federigo Enriques ("Sur quelques questions soulevées par l'infini mathématique," *Rev. de métaphysique et de morale,* March, 1917, Vol. XXIV, pp. 149-164) points out that experience, when it is idealized by reason, puts before us two kinds of infinity, the actually and the potentially infinite; suppose then "that we are potentially given by thought an infinity of objects, the question arises as to whether there is any reason to consider as logically defined a new object of thought which expresses the totality or the limit of these objects even when they are not constructed with respect to a concept of the kind which we suppose to be given *a priori.*" The answer to this question depends on a fundamental tendency of the mind; it will be negative or in some degree positive according as we feel ourselves borne toward nominalism or toward realism. Realist doctrine—at least in mathematics—in its first historical form rested on the assumption that a simple passage to the limit was always possible, and this was gradually destroyed by the progress of the infinitesimal calculus. The realist doctrine in its second historical form rests on the principle that (p. 159) "every infinity of virtually defined objects may be considered as a totality forming a class and constituting a new logical object." As distinguished from realism in its first form, in this new realism we conceive that the properties of the new object are absolutely new and that thus we cannot state them *a priori* by an induction extended from the finite to the infinite. This new form of realism is due, in its mathematical form, to Georg Cantor, but (p. 159) "the philosopher B. Russell has developed in the widest sense the philosophical consequences of the realism thus introduced into mathematics." The various paradoxes of mathematical logic have led to the conclusion that there are, in certain cases, no such things as classes of perfectly definite objects; and this realism, in its second form, is partially unsuccessful. On p. 163 we read that "the principle of an infinite number of choices is adopted by Russell and by Zermelo," and so we are apparently again forced to the conclusion that Enriques is quite unaware of the tendency shown by Russell's work published since 1905. Since the question is rather important, perhaps the present reviewer may be forgiven for dwelling on some work on the paradoxes in question since 1903.

In Russell's *Principles of Mathematics* (Cambridge, 1903) there was not any definite suggestion that the concept of class should be restricted, though there was certainly a more or less vague feeling that some classes should be excluded (see *Monist,* January, 1912, Vol. XXII, pp. 153, 157-158; and January, 1917, Vol. XXVII, p. 144). The merit of perceiving that a restriction was necessary and of attempting to give a criterion to decide which classes were legitimate seems due to Jourdain, in a paper published in 1904 (see *Monist,* January, 1917, Vol. XXVII, pp. 148-150). The question as to the *being* or *not-being* of a class is totally different from the question of the possibility of an infinite series of acts of selection,—which, by the way, neither was nor is assumed by Russell, though it is believed in by Zermelo and many others. The merit of being the first to publish an explicit recognition of the postulate involved in this assumption is due to Zermelo in 1904, and the questions relating to Zermelo's axiom have been frequently confused with, for example, Jourdain's "proof" by even eminent people; though both Russell and Jourdain pointed out repeatedly that the questions involved are quite different. In 1905 and later Russell published papers gradually showing how it was possible to work through a great deal of Cantor's theory without assuming that there are such things as classes at all, and a thorough exposition of this theory is one of the most important parts of Whitehead and Russell's *Principia Mathematica* (Vol. I, Cambridge, 1910). Thus it is obvious that this theory of Russell's not only makes the considerations of Jourdain and some others quite superfluous, but also makes such criticisms as that of Enriques entirely off the point (cf. *Monist,* January, 1917, Vol. XXVII, pp. 145-148).

This historical sketch is also relevant to our consideration of a recent paper by Dmitry Mirimanoff ("Les antimonies de Russell et de Burali-Forti et le problème fondamental de la théorie des ensembles," in *L'enseignement mathématique,* 1917, Vol. XIX, pp. 37-52). The author remarks (p. 48) that we can find in the works of Bertrand Russell, Henri Poincaré, and Julius König (*Neue Grundlagen der Logik, Arithmetik und Mengenlehre,* Leipsic, 1914) "a profound logical and psychological analysis of the Cantorian antinomies and of the notion of class," but that he "will not have any need of this analysis for the end which" he has in view. His article is characterized by the following quotation from p. 38: "People believed, and it seemed quite evident, that the existence of individuals necessarily implies that of the class of them, but Burali-Forti and Russell showed by different examples that a class of individuals may be non-existent, although the individuals exist. As we cannot refuse to accept this new fact, we are obliged to conclude from it that the proposition which seemed evident and which was believed always to be true is only true under certain conditions. And then arises the problem which we may regard as the fundamental problem of the theory of aggregates: What are the necessary and sufficient conditions for the existence of a class of individuals?" On this confusion between the ideas of existence and entity, see the article quoted above in *The Monist* for January, 1917. The author gives a solution of this problem for the particular case of classes that he calls "ordinary" classes, and his deductions rest on three postulates (p. 49) which are applied by some in the study of problems of the theory of aggregates. Further, the examples of Russell and Burali-Forti are modified (pp. 39-48) in a way that seems advantageous to the author, and the author announces (pp. 39, 52) his intention to give in a future article the reasons which determined him not to adopt in this paper the theory of König. The criterion which the author arrives at (pp. 48-52) for deciding whether individuals have a class or not is practically that suggested by Jourdain in 1904: individuals have a class if they can be arranged in a segment of the series W of all ordinal numbers and not if they cannot be so arranged. It is not worth while to enter into a criticism of this suggested criterion, which in any case has become quite superfluous through the work of Russell referred to above. Mirimanoff (p. 52) regrets that it has been impossible for him to become acquainted with work that has appeared since the beginning of the war. If he had read—which he nowhere gives any sign of having done—the works referred to above of 1904 to 1910, we do not think that it would have been necessary to write this paper.

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In the *Revue de métaphysique et de morale* for January, 1917, there is an address given by the late Victor Delbos on the general characteristics of French philosophy. A part of the late Louis Couturat's *Manuel de logistique,* which he wrote about 1906 but which is not yet published, is printed. These extracts form the greater part of the second chapter of this book, and are on the logical relations of concepts and propositions. An interesting fact about them is that the work of Frege seems to have influenced Couturat to a greater extent than was the case with Couturat's *Principes* of 1905. This contribution is fairly elementary, and does not deal with those paradoxes which are, perhaps, of the greatest interest to logicians, although it just mentions them. F. Colonna D'Istria writes on the logic of medicine according to Cabanis's *Rapports du physique et du moral de l'homme.* Arnold Reymond makes a critical study of the new and recast edition of Edouard Claparède's *Psychologie de l'enfant et pédagogie expérimentale* (Geneva, 1916). Thomas Ruyssen writes on "an idea in peril: humanity, humanitarianism, humanism." Finally there are obituary notices of Théodule Ribot (1839-1916), the eminent psychologist, and Henri Dufumier.

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In the number of the *Revue de métaphysique et de morale* for May, 1917, A. Espinas deals with the initial idea of the philosophy of Descartes, and the late Victor Delbos's lecture on method in the history of philosophy forms the second of his three lectures on the history of philosophy. A manuscript by the late Louis Couturat, which was certainly written before 1902 and which will not form part of the projected *Manuel de logistique,* is printed here and is on the algebra of logic and the calculus of probabilities. Finally Alessandro Padoa has a paper on the consequences of a change of primitive ideas in any deductive theory whatever.

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In a paper on the "infinite numbers" which Bernard le Bovier Fontenelle tried to introduce in his *Eléments de la géométrie de l'infini* (Paris, 1727), Branislav Petronievics ("Sur les nombres infinis de Fontenelle," *Rendiconti della R. Accademia dei Lincei* [*Classe di scienze fisiche, matematiche e naturali*], Vol. XXVI, 1917, pp. 309-316) tries to show that this "first attempt at a rational theory of infinite numbers," although it is obviously full of contradictions which were at once pointed out by MacLaurin and others, possesses a historical value when compared with the theories of Cantor and Veronese. Cantor's theory has, says Petronievics, an arithmetical starting-point, while that of Veronese has a geometrical one; and Veronese establishes that there is no point on an infinite straight line which corresponds to the first transfinite ordinal number of Cantor, so that "geometrical application of the transfinite numbers of Cantor is not possible." In spite of the fact that Fontenelle introduced his "infinite number of all finite numbers" as "the last one" of this series itself, Petronievics maintains that the theory of Fontenelle has a historical value in that it "may be regarded as the common source of the theories of Cantor and Veronese," and that "it is not impossible to suppose, in view of the likenesses between the theories, that Cantor and Veronese both arrived at establishing the principles of their theories when trying to avoid the flagrant contradictions into which Fontenelle fell."

There does not seem to the reviewer to be the smallest ground for supposing that Cantor was led to his theory either by reading Fontenelle or by setting out deliberately to generalize arithmetic. Indeed, one of the points of the long introduction to the translation of Cantor's later papers published under the title of *Contributions to the Founding of the Theory of Transfinite Numbers* (Chicago and London, 1915) is to show that Cantor was compelled to generalize the idea of number as a consequence of the natural development of his process of "derivation" of geometrical point-sets. In this extension it appeared clearly that the transfinite numbers began *beyond* the whole series of finite numbers in opposition to Fontenelle's notion mentioned above. Fontenelle says on page 30 of his book: "We must not be frightened at the words 'last term' in this connection. It is a last *finite* term that the natural series of numbers has not, but not to have a last finite term is the same thing as to have a last infinite term." This is a charming way of turning a universal negative proposition into a particular affirmative one. It seems that the first time that Cantor spoke more or less publicly of Fontenelle's theory was in a letter of 1886 (cf. *Zur Lehre vom Transfiniten,* Halle, 1890, p. 50), and therefore long after he had founded his absolutely different theory. That hearing of one theory may have been the psychological cause of Cantor's thinking about a fundamentally different theory is of course both possibly and probably irrelevant, but there is no ground for supposing that even this happened. It is a mistake to say that ordinal numbers cannot have geometrical applications: an illustration of the way such numbers can appear is given by this: To the series on the x-axis formed by the points 1, 1/2, 1/3, ..., $1/n$,...., the point 0 bears exactly the same relation as the first transfinite ordinal does to the finite ordinals in order of magnitude. For Cantor's remarks on Veronese and Veronese's reply, we may quote the above

Contributions (pp. 117-118). A purely analytical exposition of the infinite and infinitesimal numbers of Veronese was given by T. Levi-Civita ("Infiniti e Infinitesimi attuali," *Atti R. Istituto Veneto,* 1892).

* * *

Florian Cajori ("The Zero and Principle of Local Value used by the Maya of Central America," *Science,* Vol. XLIV, 1916, pp. 714-717) draws attention to the fact, hitherto apparently unnoticed by mathematicians, that the Maya of Central America and southern Mexico seem to have used a symbol for zero and the principle of local value much earlier than any one else. The material for Cajori's remarks is furnished by *An Introduction to the Study of the Maya Hieroglyphs,* by Sylvanus Griswold Morley (Bulletin 57 of the Bureau of American Ethnology, Washington, 1915). The early Babylonians possessed the principle of local value, but so far as we know did not possess a zero. About 200 B. C. they did have a symbol for zero, which, as Smith and Karpinski (*Hindu-Arabic Numerals,* Boston, 1911, p. 51) say, was "not used in calculation, nor does it always occur when units of any order are lacking." They did not employ it systematically in writing numbers and not at all in performing computations. The Hindus certainly did not use their symbol for zero systematically before probably the sixth century A. D., and the earliest undoubted occurrence of zero in Indian numerals is A. D. 876 (cf. also G. R. Kaye, *Indian Mathematics*, Calcutta, and Simla, 1915, p. 31, for the date of the appearance of the principle of local value in India). Now, it seems that the Maya used the zero and the principle of local value at the beginning of the Christian era if not much earlier. "As far as is known, the Maya used their numeral systems only in the counting of time as it arose in their calendar, ritual, and astronomy." Of the several Maya numeral notations the one which is of greatest interest as embodying the principle of local value and the symbol for zero is found in Maya codices but not in their inscriptions. The number system was vigesimal, with the solitary break that 18 (and not 20) *uinals* make 1 *tun*, and the symbols 1 to 19, both inclusive, are expressed by bars and dots. Each bar stands for five units and each dot for one unit, and the dots are written above the bars. Thus 19 is written as three bars above one another and four dots on the top. "The values of the bars and dots are *added* in each case. The zero, which plays a leading part in the notations found on inscriptions as well as those on codices, is represented in the codices by a symbol that looks roughly like a half-closed eye.... In writing 20... the principle of local value enters for the first time. It is expressed by a dot placed over the symbol for zero. The numerals are written, not horizontally, but vertically, the unit of the lowest order or value being assigned the lowest position. Accordingly, 37 was expressed by the symbols for 17 (three bars and two dots) in the *kin* [units] place and one dot, representing 20, placed above the 17, in the *uinal* place. The number 300 is expressed by three bars drawn above the symbol for zero ($3\times5\times20=300$). The largest number which can be written by the use of only two places or positions is $17\times20+19=359$. To write 360, the Maya drew two zeros, one above the other, with one dot higher up, in third place. Using three places to represent *kins, uinals,* and *tuns,* they could write any number not larger than 7199. Proceeding in this way the Maya wrote numbers in very compact form. The highest number found in codices is 12,489,781.... The symbols representing this number occupy six different places, one above the other.... The second numeral notation that was fully developed and employed by the Maya is found in their inscriptions. It employs the zero, but not the principle of local value. Special glyphs are employed to designate the different units. It is as if we were to write 1203 as: '1 thousand, 2 hundreds, 0 tens, 3 ones.'"

The question as to the origin of the arithmetical notation that we call "Hindu-Arabic" has received a new and unexpected contribution from Carra de Vaux ("Sur l'origine des chiffres," *Scientia,* Vol. XXI, 1917, pp. 273-282.) With the Arabs these figures are called *hindi* and the usual meaning of this word is "Indian." Now, the Arabian historian Masoudi, writing in 943 A. D., said that the Hindu numerals were discovered by a congress of wise men gathered together by the powerful and wise king Brahman under whom arts and sciences flourished. "People who are even slightly familiar with the history of philosophy will recognize this at once as a neo-Platonic legend," and a mention of the "Era of the Creation" allowed de Vaux to conclude that this legend is Persian, for that is a Persian era. Also in the work of the other Arabic historian, Albîrouni, we have a remark that the numerals came from India, that is vague and contrasts strongly with his usual exactness. This seems to show a lack of definite knowledge on Albîrouni's part.

The author then examines the word *hindi,* and comes to the conclusion that it is a form of *hindasi* whose root is the Persian *ènd* and which means metrical or arithmetical. Thus "signs of *hind*" means "arithmetical signs" and not "signs of India." Consider this example: Apollonius of Perga, who was not an Indian, was said to be *el-hindi* in some Arabic manuscripts, thus this word must evidently be translated as if it were *el-hindasi*, the geometer or engineer. It is to be noticed that in Arabian treatises the abacus is called *takht* which is a Persian name. Thus de Vaux concluded that the numerals originated in the Greek world, and the history of their slow diffusion is easier to explain if we admit that they are a neo-Platonic "or (*soit*) neo-Pythagorean" invention, for the Pythagoreans are well known to have had a taste for secrecy. From Greece the numerals passed to Persia and the Latin world, and from Persia to India and afterward to Arabia.

The figures themselves were not formed from letters of the alphabet, but directly by means of very simple conventions. These characters are due to the neo-Platonists and were known in the schools of Persia before they were in Islam, and it is there that the Arabs found them. From Persia also they passed into India.

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In the first number (January, 1917) of Vol. XXIV of the *American Mathematical Monthly*, the official journal of the Mathematical Association of America, there is an important paper by Edward V. Huntington on "The Logical Skeleton of Elementary Dynamics" (pp. 1-16). The object of the article is to outline the logical structure of elementary dynamics. Any logically developed science must begin with undefined concepts, in terms of which all the other concepts of the science are expressed; and in this case Huntington takes them to be: (1) Space and time, with the derived concepts of velocity and acceleration; (2) Forces, "as suggested by the tension and compression in our own muscles" (p. 1) and "as measured by a spring balance" (p. 3); and (3) Inert material bodies, on which our forces act. The unproved propositions, from which all the other propositions of the science are derived, are only four in number. The first is (p. 4) that "a free particle, when acted on by a force, acquires an acceleration in the direction of the force; furthermore, if a given particle is acted on at different times by two forces F and F′, and if a and a' are the corresponding accelerations, then $F/F' = a/a'$; that is, the accelerations are proportional to the forces." This principle "is best regarded as a scientific hypothesis, the truth of which has been abundantly verified by experiment." It contains the answer to the fundamental question of dynamics: "If a force gets hold of a free particle, and proceeds to act on it, what happens to the particle?" The second and third principles, which cover the case of the particle being acted on by several forces simultaneously, are the principle of the vector addition of forces and the principle of the independence of two perpendicular forces (p. 5). For any given body the ratio of the force to the acceleration produced is constant, and the value of this ratio "is a characteristic of the body, which may be called its inertia" (p. 4). "The *weight* of a body in a given locality with respect to a given frame of reference is best defined as the force required to support the body at rest with respect to that frame in the given locality" (p. 5). We then have the theorem that "if W is the weight of a body in a given locality and g is the falling acceleration of that body in the same locality, then the ratio W/g is independent of the locality, and is a correct expression for the inertia of the body" (p. 6). The proof for the case of fixed axes follows immediately from the first two principles, and a proof for the case of moving axes "belongs later in the course." The theorem (p. 6) that, in any given locality, the falling accelerations of all bodies are equal, "can be proved from general considerations; or, if preferred, it may be accepted as an empirical fact." The words "mass of three pounds" are taken (p. 7) as meaning the same thing as that the body in question "has a weight of 3 lbs." in the standard locality; the weight being a multiple of the unit of force (lb. in the British system). The fourth and last fundamental principle is the principle of action and reaction (p. 8): "When two particles are in contact with each other, or attract or repel each other according to any law like that of gravitation or magnetism, the interaction between them may be represented by a pair of twin forces, equal in magnitude and opposite in direction—one of the twins acting on one particle and one on the other, along their joining line." The definition of the "centroid or center of mass" is as a "weighted average" (p. 8), and the theorem on the motion of the center of mass is then proved. It is emphasized (p. 11) that the difficulties outside the four fundamental principles are of a mathematical sort. It will be seen that the system is based on fundamental units of force, length, and time instead of on units of mass, length, and time, and the author shows by tables (pp. 15, 16) the higher practical value of the system of derived units advocated by him. This is "one of the best arguments in favor of the use of force rather than mass as the principal undefined concept of dynamics. The only reason why the text-books so insistently base their derived units on mass instead of on force is apparently that a standard lump of metal is easier to preserve in a museum than a standard spring balance. But this is no argument for the logical priority of mass over force. As a matter of fact, the fundamental unit of force is as easy to preserve as the fundamental unit of mass, though the method of doing so does not consist in simply storing away a spring balance" (p. 16). The name of the unit of force, in the British system, is "pound" (lb.) and is defined as "the force required to support a carefully preserved lump of metal, called the 'standard pound avoirdupois,' in vacuo, in the standard locality" (p. 14).

The reviewer would remark that, though mass under the name "inertia" is a derived unit in the system advocated by Huntington, we have to use the unit of mass as a practical means of preserving the unit of force. It is quite true that this fact is no argument for the logical priority of mass: it is merely a question of practical convenience. But in either of the two systems there seem to be, at first sight, three fundamental undefined units, and so, from a logical point of view, nothing is gained by replacing mass by force as a fundamental unit. But let us look at the matter more closely. As we have learned from the work of Mach (see, e. g., his *Mechanics*, 3d edition, Chicago and London, 1907, p. 243), "mass-ratio" can be defined in terms of the mutual accelerations of bodies, and so there seems to be a logical advantage in the system in which force is not regarded as fundamental, but is defined. Further, even in Huntington's system, "force" can be defined by the property that F/a is constant, and then his system and Mach's seem to be identical. By the way, the forces we use in dynamics are not all "suggested by the tension and compression in our own muscles": the attraction of the sun is not; and it is both logically objectionable and rather confusing to a student to have various concepts with a single name.

It must be added that stress is (pp. 7-8) rightly laid on the difficulty which beginners have in realizing that, when a particle describes a curve, there is actually an acceleration along the normal.

There is an interesting review of Florian Cajori's *William Oughtred* on pp. 29-30 written by Louis C. Karpinski, where it is stated that Cajori's remark that Napier was the first to use a decimal point (1616 and 1617) is an error: it was first used by Pitiscus in 1612.

In the February number is a short account (pp. 54-55) of Cajori's presidential address to the annual meeting of the Mathematical Association of America in New York City at the end of 1916 entitled "Discussions of Fluxions from Berkeley to Woodhouse." This address was a shortened account of a book by Cajori which will appear before very long in the "Open Court Classics of Science and Philosophy." At a meeting of the Council it was decided, among other things, to consider the question of possible assistance for the *Revue semestrielle* and the *Jahrbuch über die Fortschritte der Mathematik* in view of the difficulties that must attend publication owing to the war (p. 64). Very much the same discussion was held by the Chicago Section of the American Mathematical Society (p. 97). David Eugene Smith, in "On the Origin of Certain Typical Problems" (pp. 65-71), has a very learned article on the history of the problems of (1) filling a cistern of water, (2) the *Josephspiel,* or the problem of the Turks and Christians, (3) the testament of a man about to die, dividing his estate, (4) the problem of pursuit.

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Frank Egleston Robbins (*Amer. Math. Monthly,* March, 1917, Vol. XXIV, pp. 121-123) gives an interesting and critical review of George Johnson's partial translation of and commentary on the *Introduction to Arithmetic* of Nicomachus, in his dissertation on *The Arithmetical Philosophy of Nicomachus of Gerasa* (Lancaster, Pa., 1916). The essay of Nicomachus is of course the earliest extant attempt at a systematization of the Greek science of theoretical, as distinguished from practical, arithmetic.

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In the *American Mathematical Monthly* for May, 1917, W. H. Bussey ("The Origin of Mathematical Induction," Vol. XXIV, pp. 199-207) points out that Moritz Cantor is mistaken about the use of complete induction both in his *Geschichte der Mathematik* (Vol. II, 2d ed., 1900, p. 749) and his note, correcting this mistake, on Maurolycus in the *Zeitschrift für mathematischen und naturwissenschaftlichen Unterricht* (Vol. XXXIII, 1902, p. 536). In the note Cantor said that he had found that Maurolycus described and used the method in his *Arithmeticorum libri duo* (Venice, 1575), and that Pascal had expressly borrowed the method from Maurolycus; Bussey shows that there is an error, in a minor respect, in Cantor's references. Bussey quotes a number of Maurolycus's theorems. In his sixth proposition, that any integer (n) added to the preceding one is equal to the "collateral odd number" (the nth odd number, $2n-1$), Maurolycus's proof, freely translated is: "The integer 2 added to unity makes the integer 3, but when added to 3 it makes an amount greater by 2 and this, . . . is the next odd integer, namely 5. Again, since the integer 3 added to 2 makes 5, which is the collateral [third] odd integer, when it is added to 4 the result will be greater by 2, that is. . . . it will be the next odd integer, which is 7. And in like manner to infinity as the proposition states." On this proof Bussey remarks (p. 201): "This is not a very clear statement of a proof by mathematical induction but the idea is there." The eleventh proposition, that every triangular number added to the preceding triangular number is equal to the collateral square number, or, in modern notation, $n(n+1)/2+(n-1)n/2=n^2$, is the one which Cantor said, in the above note, Pascal got from Maurolycus and which Maurolycus proved by complete induction. But Cantor is mistaken in saying that this theorem is proved by complete induction: the first undoubted case of a proof by complete induction is the fifteen proposition, that the sum of the first n odd integers is equal to the nth square number. Maurolycus's proof is (p. 203): "By a previous proposition the first square number (unity) added to the following odd number (3) makes the following square number (4); and this second square number (4) added to the third odd number (5) makes the third square number (9); and likewise the third square number (9) added to the fourth odd number (7) makes the fourth square number (16); and so successively to infinity. . . ." Pascal mentioned in a letter to Carcavi the fact that he borrowed from Maurolycus, and he repeatedly used the method of complete induction in connection with his arithmetical triangle and its applications. Bussey then gives two interesting examples of Pascal's use of the method of complete induction, and finally gives some other and more recent uses of it.

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In the same number of the *Monthly,* David Eugene Smith ("Mathematical Problems in Relation to the History of Economics and Commerce," pp. 221-223) maintains that "a very good history of civilization could be written from the wide range of problems of mathematics." In the subject of commercial and economic history, for example, he mentions that the problems in the manuscripts and early printed books on arithmetic in the fifteenth century tell us that Venice was then the center of the silk trade, although Bologna, Genoa, and Florence were then prominent; the problems also tell us the cost of the luxuries and necessities of life; the rent of houses; the changes in commercial customs and the rise in standards of business integrity. "Not only to the economist and the student of commerce is the field a rich one, but it is well worth the study of any one who may be possessed of doubt as to the relation of mathematics to the daily life of the race. Not only can the history of the problem easily be made the history of commerce and economics, but the history of mathematics can easily be made the history of civilization."

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In the number of the *Bulletin of the American Mathematical Society* for March, 1917 (Vol. XXIII), there are two interesting papers by Edward V. Huntington on the logical postulates for order. In "Complete Existential Theory of the Postulates for Serial Order" (pp. 276-280) Huntington establishes the "complete independence"—in the sense defined by E. H. Moore of Chicago in his *Introduction to a Form of General Analysis* of 1910—of each of three different sets of postulates for serial order. The first set is new and very convenient for many purposes; the second set dates back to Vailati (1892); the third set is a modification of the second set and was introduced in Huntington's well-known paper on "The Continuum as a Type of Order" in the *Annals of Mathematics* for 1905. In "Complete Existential Theory of the Postulates for Well Ordered Sets" (pp. 280-282) Huntington gives three sets of independent postulates for well-ordered systems, each of these three sets being "completely independent" in the above sense. R. L. Borger ("A Theorem in the Analysis of Real Variables," pp. 287-290) gives a theorem on two real functions of two real variables which is derived from a theorem in Kowalewski's *Die komplexen Veränderlichen und ihre Funktionen,* and deduces from it the exceedingly fundamental and important theorem that if any function of a complex variable possesses a finite derivative at each point of a simply connected closed region, then this derivative is continuous, all the derivatives of the function exist, and the function may be represented by a power-series. Mathematicians who are acquainted with the nature of the progress brought about by Goursat's proof of Cauchy's theorem will at once see how important this note is. J. R. Kline ("Concerning the Complement of a Countable Infinity of Point Sets of a Certain Type," pp. 290-292) proves a theorem which is a general case of the theorem proved by Hausdorff in his *Grundzüge der Mengenlehre* of 1914 that, if E denotes a Euclidean space of two or more dimensions while R is an enumerable set of points belonging to E, then E – R is a connected set. Kline's theorem was proved by Robert L. Moore (*Trans. Amer. Math. Soc.* for 1916) on the basis of a system of axioms proposed by him.

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The number of the *Bulletin of the American Mathematical Society* for May, 1917 (Vol. XXIII, No. 8), contains several articles of interest to those who cultivate the philosophical and historical aspects of mathematics. Samuel Beatty ("The Inversion of an Analytic Function," pp. 347-353) proves the existence of the inverse of an analytic function when the conception of an analytic function which is due to Goursat is the starting-point. In the theory of Weierstrass this proof is made to depend on the representation by a series of powers and in Cauchy's theory on the Jacobian of the real and imaginary parts of the function with reference to the real and imaginary parts of the variable. It is well known that Goursat showed in 1900 how the fundamental proposition on complex integration in Cauchy's theory could be proved merely from the assumption that the function in question has a finite derivative at each point of a simply connected domain, without any assumption of the continuity of this derivative. This continuity was then proved as a consequence of the Cauchy-Goursat theorem. The method of Beatty's proof makes use of the theory of sets of points. Thomas S. Fiske ("Emory McClintock," pp. 353-357) gives a biography of Emory McClintock (1840-1916). McClintock's first paper on pure mathematics entitled "An Essay on the Calculus of Enlargement," in the *American Journal of Mathematics* for 1879 "was an effort to present the theory of finite differences and the differential calculus from a unified point of view. The paper may be regarded as a precursor of recent attempts to consider difference equations as differential equations of infinite order. His other more important papers were a series of researches on solvable quintic equations published in the *American Journal of Mathematics* [for 1884, 1885 and 1898] and a paper on the theory of numbers ['On the Nature and Use of the Functions Employed in the Recognition of Quadratic Residues'] published in the third volume [1902] of" the *Transactions of the American Mathematical Society* (p. 355). "When one considers that McClintock made no use of the powerful labor-saving machinery which has revolutionized modern analysis, the results obtained by him in his researches on quintic equations, as well as some of his other achievements, appear to indicate a truly wonderful power of manipulation and clearness of vision" (p. 356). A list of McClintock's mathematical publications is given.

J. H. Weaver ("On Foci of Conics," pp. 357-365) gives (1) a short historical sketch of the development of the properties of conics connected with the foci, and (2) some of the theorems from Pappus which have a bearing on foci and tangents. According to Zeuthen (*Geschichte der Mathematik im Alterthum und Mittelalter,* Copenhagen, 1896, p. 211) it seems that the focus for the parabola may have been known to Euclid. However, we have no mention of such points or of any of their properties until the work of Apollonius on conics (Book III, Probs. 45-52), but Apollonius did not use or mention in any way a focus for the parabola. Pappus gave the first recorded use and proofs of the focus-directrix definition of conics. Johann Kepler named the points in question in a work of 1604, and part of the short account of the conic sections that he gave (*Opera Omnia,* ed. Frisch, Frankfort, 1859, Vol. II, p. 185) is freely translated by Weaver (p. 359) as follows: "There are among these curves certain points of especial consideration, which have a certain definition but no name, unless they usurp for name the definition of some property. For if from these points lines are drawn to the points of contact of tangents to the section, these lines make equal angles with the tangents....We, because of the properties of light and the eye, from the viewpoint of mechanics shall call these points *foci.* We might have called them *centers,* because they are on the axis of the section, if authors, in the hyperbola and ellipse, were not accustomed to calling another point the center. In the circle there is one focus, the center. In the ellipse there are two foci equally distant from the center, and more removed in the more acute. In the parabola, one focus is within the section and the other may be considered either within or without the section and removed to an infinite distance from the first focus, so that if a line drawn from this *caecus* [blind] focus to a point of the section will be parallel to the axis. In the hyperbola, the external focus becomes nearer the internal focus as the hyperbola becomes more obtuse." The method of the work of Kepler was developed and added to by Desargues, and important work on foci was done by Maclaurin, Poncelet, Plücker, and many others.

Finally, there is a short and extremely interesting paper by Jekuthial Ginsburg ("New Light on Our Numerals," with an introductory note by David Eugene Smith, pp. 366-369). That our common numerals are of Hindu origin seems to the author to be a well-established fact, and that Europe received them from the Arabs seems equally certain, but how and when these numerals reached the Arabs is a question that has never been satisfactorily answered. The article calls attention to a paper by the French orientalist F. Nau in the *Journal asiatique* foi 1910 (Series X, Vol. XVI) showing that the Hindu numerals were known to and appreciated by the Syrian writer Severus Sebokht who lived in the second half of the seventh century; that is, about one hundred years before the date of the first definite trace that we have hitherto had of the introduction of the system into Bagdad. Sebokht says, after asserting that the Greeks, in astronomy, were merely the pupils of the Babylonians: "I will omit all discussion of the science of the Hindus, a people not the same as the Syrians; their subtle discoveries in this science of astronomy, discoveries that are more ingenious than those of the Greeks and the Babylonians; their valuable methods of calculation; and their computing that surpasses description. I wish only to say that this computation is done by means of nine signs. If those who believe, because they speak Greek, that they have reached the limits of science should know these things they would be convinced that there are also others who know something" (p. 368). On this fragment Ginsburg remarks (pp. 368-369) that it "clearly shows that not only did Sebokht know something of the numerals, but that he understood their full significance, and may even have known the zero as Rabbi ben Esra did, in spite of the fact that he, too, speaks of nine numerals." However, Smith (pp. 366-367) remarks that the article "shows that the zero was probably not in the system as then mentioned, showing at least that its value was not generally comprehended in the seventh century and possibly confirming the impression that the symbol had not yet been invented." With regard to the question as to how Sebokht could have obtained information about the Hindu numerals, Ginsburg remarks (p. 369) that the city where Severus lived, in the northeast part of Mesopotamia, "was situated in a rich and fruitful country, was long the center of a very extensive trade, and was the great northern emporium for the merchandise of the east and west;" and "the exchange of goods is always accompanied by the exchange of ideas." Further, the weight of the evidence is (p. 369) in favor of Sebokht's work being at least one of the agencies by means of which the knowledge of the numerals was transmitted to the Arabs.

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Raphael Demos ("A Discussion of a Certain Type of Negative Proposition," *Mind,* Vol. XXVI, 1917, pp. 188-196) applies to particular negative propositions the treatment which Bertrand Russell (cf. Russell and Whitehead's *Principia Mathematica,* Vol. I, Cambridge, 1910) has applied to "descriptive phrases" or "incomplete symbols." Russell "found himself confronted with the fact that to accept descriptive phrases as significant in their given form would be to people the world of things with the apparent objects of such self-contradictory and fantastic descriptions as 'round-square,' 'centaur,' etc."; and Demos "was faced with the fact that to accept negative propositions at their face value would be to people the world of objects with negative facts, a type of objects which experience fails to disclose." Demos, somewhat like Russell, by viewing the negative proposition as an incomplete symbol, was led to declare it meaningless in its apparent form, and its apparent object —the negative fact—to be nothing. In this article he stated, first, that a particular simple negative proposition is an objective entity whose peculiarity as negative is not dependent upon the mind's attitude toward it. He then argued that the negative proposition cannot be construed in the form which it apparently possesses, inasmuch as such construction would make it formally different from positive propositions and would endow it with purely negative objects, which are, it seems, nowhere to be found in experience. He concluded that some special interpretation must be given to the negative proposition, and showed that its negative element is a modification, not of any distinct constituent (such as the predicate) in the proposition, but of the whole content of it. Thus any negative proposition is a modification, in terms of "not," of the rest of its content, and—since the latter is positive—a modification of some particular positive proposition. He stated the meaning of "not" to be "opposite"—a relational qualification in terms of the familiar relation of opposition or contrariety among positive propositions—and hence the meaning of the whole proposition "not-p" to be "opposite of *p*." He argued that, so stated, a negative proposition is an ambiguous description of some positive proposition, and that, completely stated, it is of the form "an opposite of *p* is true," or "some *q* is true which is an opposite of *p*." Thus he defined a particular simple negative proposition as an ambiguous description of some true positive proposition in terms of the latter's relation of opposition to a certain other positive proposition, such that, in terms of the former, reference is achieved to the latter. Lastly, he explained that negative knowledge is knowledge of a true positive proposition by description in terms of its opposition to some other proposition, and hence must be characterized as positive in reference but not in content, inasmuch as the proposition referred to is not a constituent of the complex of assertion or knowledge. "Substantially the above definition of simple negative propositions applies to double and '*n*-ple' negatives as well; the latter, too, are descriptions of positive propositions which are true in terms of what they oppose. There is this difference, however, that whereas simple negatives are functions of a positive content, double and other negatives are functions of a negative content, such that any negative proposition in the *n*th power is a function of a content which is negative in the $(n-1)$th power." Φ

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C. E. Hooper publishes "The Meaning of the Universe" (*Mind*, April, 1917), the first instalment of an article of massive appearance. The definition is as follows: the *Universe* means the totality of real thought-objects (or object-matters) considered under four related aspects: (1) space, (2) time, (3) the variety in unity of natural characters, i. e., real thought-objects as particulars having natures of their own, but natures agreeing in various specific and generic respects with the natures of other particulars, (4) unity in variety of natural causation. Time and space are both objective. Mr. Hooper goes on to define thought-object, reality and aspect. A thought-object is apparently an *intended* object, whether or not a reality corresponds to the intention (e. g., Kant's *noumenon*). Reality is contrasted not with appearance but with "mental figment," and includes subsistent as well as existent objects. It is difficult to tell how far the term "thought-object" has an idealistic bias in Mr. Hooper's mind, but reality, at all events, seems to be merely a sum or system of objects which are severally real. The universe is thus a real thought which contains all other real thought-objects in their manifold relations. *Symbolic* entities (ideas, signs) are comprehended, but whether *per se* or only as reflected upon (made objects of thought) is not stated. It would seem that imaginary or inconceivable thought-objects, such as Meinong's pets, the golden mountain and the round square, are to have no place in the universe, but are discarded as "figments." While the universe contains finite thought-objects and symbols, it does so only in fact, not in nature. Of the four modes or aspects, space and time may be classed together as *coincidentals,* while the systems of natural characters and natural causation may be termed *co-essentials.* On the other hand space and nature may be classed together as static, time and causation as dynamic. We find some difficulty in understanding how Mr. Hooper accounts for the universe's being known at all. "It cannot, like a finite object, be actually related to some fellow object. It is as related to the mind or system of subjective ideas that we know all that is possible to know about it." But the "mind" if genuinely symbolic is a thought-object; and if the universe cannot be related to a finite object which is part of itself we do not know how it can be related to the mind. But criticism of so substantial an article should be deferred until its completion in succeeding issues. η

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Agnes Cuming ("Lotze, Bradley, and Bosanquet," *Mind,* April, 1917) declares Lotze's logic to be a partial revolt against the intellectualism of Hegel. Our intelligent experience, according to Lotze, is only a small part of the real world and thought is only a small part of our intelligent experience. Thought is a tool, a substitute for adequate perceptive intuition. Bradley's and Bosanquet's logic are similar in so far as each is influenced by Lotze. They hold an almost identical definition of "idea," and agree in their theories of judgment. Bradley however arrives at reality ontologically and Bosanquet epistemologically. "Knowledge for Bosanquet is the system of reality progressively demonstrated before our eyes.... In this emphasis on system as the postulate of knowledge....Bosanquet is in advance of Bradley." Lotze insists on feeling as a criterion, and is thus very far from Bosanquet with his conception of system, but he admits the essential of Bosanquet's position, which is the inadequacy of feeling. Lotze is a dualist: he divides sharply the feeling which supplies the material from thought, exercising a formal activity upon it. In Bradley the dualism becomes a gloomy scepticism; thought and its object are forever sundered. But Bosanquet bridges the gulf. In both Bosanquet and Bradley the separation of thought and reality is inherited from Lotze with his idea of the "scaffold" of thought. The only possible criterion of knowledge is immanent—a criticism of a lower from a higher point of view. Miss Cuming considers that Bosanquet has improved upon both Lotze and Bradley; the direction which she believes to be progress seems to be almost a return to more orthodox Hegelianism.

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In the same number B. M. Laing ("Schopenhauer and Individuality") considers that Schopenhauer fails to appreciate the metaphysical claims of individuality. He dethrones reason, making it a mere temporary organ of the will. He interprets Kant in such a way as to make Kant assert that the mind creates the world of things, instead of merely conditioning it. This perversion of the Kantian doctrine leads Schopenhauer to hold (in contrast to Kant) that the world of space and time is an illusion. Hence he is unable to conserve individuality. and tends to confuse individuality with (temporal and spatial) individuation. Schopenhauer's monism is a mere prejudice against multiplicity, and his will a pure abstraction. Furthermore, he confuses the will with bodily wants and cravings. Schopenhauer exposes himself on every side to such destructive criticism: but while Mr. Laing seizes upon some of his weakest points in his interpretation of Kant, the view of individuality which Schopenhauer represents, and which is more abiding then Schopenhauer, cannot be said to be demolished. η

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Scientia for February, 1917, opens with an article by Gino Loria on the history of imaginary numbers. He takes as his text Kronecker's aphorism "Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk." There is no square root of a negative quantity, said the Viga Ganita, for it is not a square, and the mysterious quantities remained an enigma and defied concrete interpretation until a memoir appeared about the end of the eighteenth century, written by an unknown Danish land-surveyor, Caspar Wessel by name. It seems to have suffered a fate like that of Swinburne's Queen Rosamund, of which not a copy was asked for or sold. Mendel's discovery remained unheeded for forty years, but Wessel's was not unearthed until a century after his death. But in this summary is no more room for comment on Loria's charming paper. P. Zeeman writes upon the hypothesis of the immovable ether, describing the experiments of Fizeau, Michelsen and Morley, Eichenwald, and himself, and concluding that it seems impossible by any imaginable means to measure absolute velocities. He points the lesson that the most important scientific principles are the results of boldly generalized experiments. J. R. Carracido discusses the foundations of biochemistry, and the lines research must follow in the pursuit of organic synthesis. He does not see why success should not be reaped before the end of the century, and success it will be, even if limited to the synthesis of the most rudimentary form of living matter.

The number for March, 1917, contains an admirable article by Gaston Milhaud, in which he attacks the very difficult problem as to the extent to which Descartes was influenced by Bacon. M. Cantone discusses the present trend of physical research in a paper surveying the work of those whose discoveries have in thirty or forty years revolutionized our outlook on the world of matter. Etienne Rabaud writes on the life and death of species. An analysis of the current doctrine of "means of defense" prepares for the question as to how it is that species persist in spite of the daily hecatombs of individuals. γ

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In *Scientia* for April, 1917, we have, from the pen of Francisco Iñiguez, of the Observatory of Madrid, a slight but interesting sketch of what we know about stellar spectra, their classification, and the light they throw on the subject of the evolution of the stars. We are warned of the limits we must set to our inferences in considering the nebulae, for what we know as yet of these celestial bodies does not justify our indulging in theories on the subject. The author then indicates how we may infer the existence of dark stars, how their evolution still continues, and points out their connection with meteorites and cosmic dust. Etienne Rabaud brings to a close his paper on the life and death of species, of which this second instalment deals with the conditions of the persistence and of the disappearance of species. He finds the affinity of organisms to be the crux of the problem. This leads to the consideration of the conditions of attraction and repulsion, and to a short discussion of parasitism and symbiosis, with the cosmic influences which, often of great complexity, determine the life of a species. The whole forms a graphic picture of the variations of the relative proportion of individuals and species. The slightest change in the conditions of normal life may lead to the disappearance of the last member of a species, or on the contrary the species may thrive and continue to thrive. Species persist, they increase immeasurably in numbers, or their numbers fall off, and they disappear, subject but to the intervention of two sets of influences, affinity and the circumstances that determine their displacements in space. "These modifications, no doubt, sometimes involve other important modifications in the conditions of life; variations may ensue which find their repercussion in the aggregate of the interaction. Thus, linked to one another and to the world from which they come, the life, the transformations, and the death of organisms are functions of their interdependence." γ

BOOK REVIEWS AND NOTES.

Diderot's Early Philosophical Works. Translated and edited by *Margaret Jourdain.* "The Open Court Classics of Science and Philosophy," No. 4. Chicago and London: The Open Court Publishing Co., 1916. Pp. vi, 246. Price $1.25 or 4s. 6d. net.

Among the documents of the renaissance of the eighteenth century, none are of more interest than the early informal contributions to ethics and philosophy of Diderot, written with much of the incoherence of the epistolary form. They are, as he claims again and again, *Letters*; and they are letters written in a hurry. *The Philosophic Thoughts,* which is the only one of Diderot's works in this selection not in the epistolary form, is said to have been thrown together between Good Friday and Easter Monday of 1746. Yet they are not philosophic journalism, no mechanical transmission of the current philosophical coin of the day. It is for their originality of outlook that they have been closely studied in Germany, while in England there is Lord Morley's study of Diderot in relation to the movement centered in the *Encyclopédie, Diderot and the Encyclopædists.*

This selection includes the *Philosophic Thoughts,* a breviary of eighteenth-century scepticism, a copy of which was found in the possession of the unfortunate La Barre, and in which Diderot appears as a Deist, to whom the argument from design (Thought XX, pp. 56-58) is still of weight: "I am greatly deceived (he writes) if this proof is not well worth the best that has ever issued from the schools." That very argument is very differently treated in the *Letter on the Blind* (p. 109) by Diderot's mouthpiece, the blind mathematician, Nicholas Saunderson, who conjectures a world in its early stages "in a state of ferment," without any vestiges of that "intelligent Being whose wisdom fills you with such wonder and admiration here.... What is our world, but a complex, subject to cycles of change, all of which show a continual